(xii.) $+0''.60 \sin(\omega-\omega')$.

Hansen's tables give	$+1''.58$
Hansen's theory gives	$+1''.33$
Delaunay gives	$+0''.87$

The evidence of the observations is therefore in favour of Delaunay.

(xiii.) I am unable to explain $-0''.31 \cos(\omega-\omega')$.

(xiv.) $-1''.18 \sin(-\Omega) - 0''.28 \cos(-\Omega)$. The coefficients are uncertain to about $0''.3$ on account of the *Mars* term just discussed. It is clear, however, that Hansen's figure of Earth terms require considerable diminution. On the other hand, the term in $\sin \Omega \cos g$ apparently requires a large increase. Hansen's tables are equivalent to $+0''.59 \sin \Omega \cos g$; I have already applied $+0''.445 \sin \Omega \cos g$, bringing the coefficient up to $+1''.04$ in accordance with Hill's calculations. On analysing the coefficients of $\cos g$ and $\sin g$, given on pp. 415, 416 (last two columns), I obtain

$$(-0''.22 \sin \Omega + 0''.29 \cos \Omega) \cos g$$
$$+ (+0''.13 \sin \Omega + 0''.05 \cos \Omega) \sin g$$

The coefficient of $\sin \Omega \cos g$ should, therefore, apparently be $+1''.26$, or about double Hansen's value.

I ought to add that any at present undiscovered term with a period between fourteen years and twenty-five years (a very wide range) will affect an analysis for Ω based upon fifty-four years. This range will shortly be considerably reduced by taking the Airy period into the discussion. As no two discussions have hitherto given the same coefficient of $\cos \Omega$, I suspect the existence of such an undiscovered term.

The Parallactic Inequality: A Reply. By P. H. Cowell, M.A.

On p. 567 of the present volume of the *Monthly Notices* Professor Turner writes: "If the solar parallax as determined from observations of the Moon is affected with an entirely unknown systematic error, then it is a pity to publish the result." I thought I had made it clear on p. 96, in a passage that Professor Turner has quoted on p. 406, that I saw no means of separating the errors of observation from the error of tabular parallactic inequality, and that my results were given on the assumption that the errors of observation were zero. I see no objection to publishing a result if its limitations are clearly set down.

I said last December, and I still say now, that I am unable to determine how much of the coefficient of the parallactic in-

equality is to be attributed to errors of observation. I did say, however, that I thought probably not more than $\pm 0''.3$. If this estimate is correct the lunar observations give the solar parallax almost free from accidental error (that is to say, more observations under similar conditions are not required), but subject to an unknown systematic error possibly amounting to $0''.02$. I still adhere to the $0''.3$ given above, partly because I have since found a term $0''.28 \cos D$, which must be attributed to errors of observation, and partly because of the investigations of which I give some account at the end of the present paper, and which were undertaken in consequence of the suggestions of Professor Newcomb on p. 570.

The fundamental idea of Professor Turner was that the observations with another instrument should be examined with a view to seeing whether they gave the same parallactic inequality as the transit circle. It will be noticed that whatever the answer to this question may be it does not tell us much. For if the two instruments agree, they may both be wrong; and if they disagree, there is nothing to show which is wrong.

Again, Professor Newcomb has suggested the exclusion of the daylight observations. I have carried out the necessary calculations with this modification of Professor Newcomb's plan, that because the six o'clock observations were rejected in summer, it was absolutely necessary to reject them in winter as well, for reasons which I set out later. It is a matter of judgment only, and not of absolute certainty, whether the six o'clock observations are so far inferior to the rest that it is better to leave them out altogether.

Following out his own idea, Professor Turner set out to examine fifteen years of the Old Altazimuth. He was looking for a discordance of possibly $0''.3$—I do not think he can have expected to find anything much larger than this. Under these circumstances he was, in my opinion, bound to keep his accidental error under a probable value of $0''.2$, or even less; but let us say $0''.2$. On p. 96 I stated that the weight of the parallactic inequality found from 5647 observations of unit weight was 515. It would therefore require 1000 observations at the least, with an instrument as good as the transit circle, to obtain a difference of parallactic inequality with a probable error of $0''.2$. And, moreover, these 1000 observations must be carefully worked up without unnecessary waste in the working. Now Professor Turner has chosen the altazimuth as his comparison instrument; he has thrown aside the observations of what he calls "day 15" (or full moon) and smoothed out days 14 and 16. These are the most important observations of all, because the parallactic inequality is then most different from its mean value, and we are at once left with an amount of material insufficient for its purpose. It was not necessary to confine the investigation to fifteen years, and astronomers, like lawyers, ought to adopt the doctrine of "best evidence."

This being my view about a comparison with fifteen years of the altazimuth, *à fortiori* I can give no credit to a result based upon four years, 1847–1850. To my judgment the figures given by Professor Turner (p. 411, Table V., column 2, $0''\cdot0$, $-1''\cdot1$, $+1''\cdot1$, $-0''\cdot2$, $-1''\cdot1$, $-0''\cdot7$, $-0''\cdot2$, $+0''\cdot7$, $+0''\cdot1$, $-0''\cdot5$) are palpably of an accidental character. He implicitly admits this himself of the two largest by smoothing them out, and I was astonished that he should have considered the smaller figures unimpeachable.

In his second paper (May 1904) Professor Turner seems in some apprehension as to whether I have mistaken a cosine term for a sine term. I can assure him he need not be in the least anxiety. His argument is based upon the assumption that we only have one quarter available; an assumption that seems to owe its origin to the fact that in his first paper he quite properly subtracted the results for any day before full moon from the corresponding day after full moon, as he was then dealing with an odd function of D. When it is a question of cosines the corresponding results should be added. If the table in which he has set out so clearly the similarity between sin D and $1+\cos$ D in the second quarter of the Moon (D is usually measured from new moon) be extended to the third quarter, it will be seen that the two functions are of opposite signs. Analytically the cross term in the normal equations, when the errors are equated to $\delta_1' \sin D + \Delta_1 \cos D$, is proportional to the mean value of sin 2D, the value of which is found on p. 94 to be $-0\cdot04$, and has been neglected throughout by me.

It was so obvious to me that no preliminary remarks would avoid the necessity of resorting to normal equations that I omitted the remarks and went straight to the equations. Perhaps now, though somewhat late, I had better give the remarks. I do so in a perfectly general form.

Let ϕ be a function whose coefficient a we wish to determine. Let χ_1, χ_2, &c., be other functions, such that they are liable to be confounded with ϕ. To give a precise meaning to this, consider the method of finding a. The errors are grouped according to the values of ϕ, which we will call ϕ_1, ϕ_2, &c. For the typical group of n_r observations we have

$$n_r a \phi_r = \Sigma e;$$

and its contribution to the normal equation is

$$a n_r \phi_r^2 = \Sigma e \,.\, \phi_r;$$

and the complete normal equation is

$$a \Sigma n_r \phi_r^2 = \Sigma e \,.\, \phi.$$

Now a function χ is liable to be confounded with ϕ if

$$\Sigma \chi \phi \neq 0$$

and it will be seen that this is a reciprocal relationship; if χ may be confounded with ϕ, so may ϕ with χ.

Having, then, chosen the function ϕ, whose coefficient I wish to investigate, I pick out all possible forms of the function χ, and the value of my analysis will depend upon the completeness with which I have picked out these functions. In particular—

when $\phi=\sin$ D we have	$\chi_1=\mu$	$\chi_2=\sin 2\text{D}$	(p. 94)
when $\phi=\cos$ D we have	$\chi_1=1$	$\chi_2=\cos 2\text{D}$	(p. 582)
when $\phi=\sin$ A we have	$\chi_1=\sin(\text{A}+\text{D})$	$\chi_2=\sin(\text{A}-\text{D})$	(p. 413)

this last illustration being, as far as I am aware, original; and it, moreover, introduces an important correction ($0''{\cdot}27$) into the value of the eccentricity when investigated by comparison with Hansen's Tables (see p. 418). On pp. 420 and 693 I have pointed out cases where it would be necessary to resort to the same process when only fifty years' observations are under discussion, but when the process can be avoided by waiting for the reduction of the Airy period, 1750 to 1851.

For simplicity I now consider one fraction χ only. The disentanglement of ϕ from χ involves of course the disentanglement of χ from ϕ. The errors are therefore equated to

$$a\phi+b\chi.$$

Now if $\Sigma\phi\chi=k\Sigma\phi^2$, it follows that a term $\beta\chi$ is liable in an analysis for ϕ alone to be mistaken for $\beta k\phi$. I therefore in the first place solve for $a+kb$ on the supposition that this is the coefficient of ϕ. Next I try to distinguish between

$$a\phi+b\chi \quad \text{and} \quad (a+kb)\phi$$

that is to say, I try from the errors to measure b considered as the coefficient of $\chi-k\phi$.

Now in most cases it is natural to take for the forms of χ and ϕ functions that vary between the limits ± 1. When ϕ, χ are liable to be confounded, it will generally happen that $\chi-k\phi$ varies between smaller limits, $\pm\lambda$ say. Writing, then, $b\lambda\,\dfrac{\chi-k\phi}{\lambda}$ instead of $b(\chi-k\phi)$, so that $\dfrac{\chi-k\phi}{\lambda}$ varies between the limits ± 1, it will be seen that $b\lambda$ can be determined with approximately the same accuracy as the coefficient of any sine or cosine term that does not present any special difficulty. Also it will be seen that, when it comes to fitting the formula found, in its form

$$(a+bk)\phi+b\lambda\,\frac{\chi-k\phi}{\lambda}$$

on to the errors, the usual degree of precision may be expected.

It is only when we exhibit the values of a and b separately, for the latter multiplying $b\lambda$ by $\frac{1}{\lambda}$, that we multiply discordances by $\frac{1}{\lambda}$ and get values with weights reduced in the ratio λ^2 to 1.

If we examine Professor Turner's first paper in the light of these remarks, it appears that there has been no adequate attempt to disentangle the quantities I call μ and δ_1. Professor Turner evidently recognises that δ_1 cannot be determined without determining μ; he omits, however, to see that the relationship is reciprocal and that μ cannot be determined apart from δ_1. His method of determining δ_1 is therefore based upon giving two alternative guesses at μ (p. 407); and it will be seen that these alternative values of μ are in reality values not of μ but of $\mu+0{\cdot}7\ \delta_1$ and of $\mu+0{\cdot}4\ \delta_1$ respectively.

I come now to the calculations that I have performed in consequence of Professor Newcomb's suggestion that the daylight observations should be omitted.

I have already stated that it thereupon became necessary to omit the six o'clock winter observations. It is for this reason. Suppose we took the December and January observations only. For these months g' is nearly zero. We are therefore unable to distinguish between

$$\sin(g-g'+\omega-\omega') \text{ or } \sin D$$

the parallactic inequality and

$$\sin(g+\omega-\omega')$$

Now on p. 419 I obtained corrections

$$-0''{\cdot}57 \sin(g+\omega-\omega')-0''{\cdot}46 \cos(g+\omega-\omega')$$

and although I then stated that the arguments were uncertain, and might be $g+\varpi$ or $g+2\varpi+3V-5E$, still the fact remains that as long as errors of this period, or approximately of this period, exist, the parallactic inequality is not to be found from observations in the months of December and January alone. And the same objection applies to any scheme whereby the observations discussed are not symmetrically arranged with regard to the argument g' or time of year.

Consequently I cut down the material to days 10–18 and 39–47 in the notation of my December paper, full moon being at $14\frac{1}{4}$ and $42\frac{3}{4}$ days approximately, so that the observations extend for four days on each side of full moon, approximately.

With the help of Table III. pp. 90–95 *Monthly Notices*, Dec. 1903 the coefficients of the normal equations can be formed on the assumption that every error is equated to

$$\mu+\delta_1 \sin D+\delta_2 \sin 2D.$$

The left-hand sides are

$$\begin{aligned} 3052\mu + 1344\delta_1 - 2097\delta_2 \\ 1344\mu + 768\delta_1 - 1144\delta_2 \\ -2097\mu - 1144\delta_1 + 1734\delta_2 \end{aligned}$$

Consider the quadric

$$a\mu^2 + b\delta_1^2 + c\delta_2^2 + 2f\delta_1\delta_2 + 2g\delta_2\mu + 2h\mu\delta_1$$

where

$$\begin{array}{lll} a = 3052, & b = 768, & c = 1734 \\ f = -1144, & g = -2097, & h = 1344 \end{array}$$

The quadric may be put into the form

$$a\left\{\mu + \frac{h}{a}\delta_1 + \frac{g}{a}\delta_2\right\}^2 + \frac{ab-h^2}{a}\left\{\delta_1 - \frac{gh-af}{ab-h^2}\delta_2\right\}^2 + \frac{\Delta}{ab-h^2}\delta_2^2.$$

where $\Delta = abc + 2fgh - af^2 - bg^2 - ch^2 = 9152000$

$$ab - h^2 = 537600$$
$$gh - af = 673120$$

Consequently the quadric becomes

$$\begin{aligned} &3052\{\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2\}^2 \\ &+ 176\{\delta_1 - \tfrac{5}{4}\delta_2\}^2 \\ &+ 17\delta_2^2 \end{aligned}$$

and therefore

$\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2$ can be determined with a weight equal to 3052 single observations

$\delta_1 - \frac{5}{4}\delta_2$ with a weight of 176

δ_2 with a weight of 17.

This is practically equivalent to saying that δ_2 cannot be determined at all from the material.

The same results follow, with approximate accuracy, from geometrical reasoning, as follows:—

By changing the sign of all second limb errors, we get a determination, with weight 3052, the total number of observations employed of

$$\begin{aligned} &\mu + \delta_1 \times (\text{mean numerical value of sin D} = 0{\cdot}44) \\ &+ \delta_2 \times (\text{mean numerical value of sin 2D} = -0{\cdot}68) \end{aligned}$$

3 D

Next, if we try to measure the difference between

$$\mu + \delta_1 \sin D + \delta_2 \sin 2D$$

and

$$\mu + \delta_1 \times 0{\cdot}44 - \delta_2 \times 0{\cdot}68$$

we find that we are approximately always measuring a multiple of $\{\delta_1 - \frac{5}{4}\delta_2\} \times 0{\cdot}4$, and that therefore δ_1, δ_2 cannot be separated.

Lastly, as the factor of $(\delta_1 - \frac{5}{4}\delta_2) \times$, $0{\cdot}4$ ranges between the values ± 1, the above quantity can be determined from 3052 observations with a weight of $3052 \div 3$ approximately, and therefore $\delta_1 - \frac{5}{4}\delta_2$ with a weight $(0{\cdot}4)^2 \times 3052 \div 3 = 162$, the more accurate weight being 176, as stated above.

I have grouped the forty-eight periods of analysis into six groups of eight, the group of eight being taken as a unit in order to get rid of inequalities with argument $\omega - \omega'$, whose period is 8×400 lunar days nearly. The results are

Periods.	$\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2$.	$\delta_1 - \frac{5}{4}\delta_2$.
86–93	$-0''{\cdot}30$	$-0''{\cdot}24$
94–101	$-0{\cdot}40$	$-0{\cdot}34$
102–109	$+0{\cdot}13$	$-0{\cdot}48$
110–117	$+0{\cdot}29$	$-0{\cdot}20$
118–125	$+0{\cdot}18$	$-0{\cdot}10$
126–133	$-0{\cdot}26$	$-0{\cdot}20$
Means	$-0{\cdot}06$	$-0{\cdot}26$

Considering the superior weight of $\mu + 0{\cdot}44\delta_1 - 0{\cdot}68\delta_2$, the discordance of the separate results is striking, and indicates marked changes in semi-diameter. On the other hand, the accordance of the six separate values of $\delta_1 - \frac{5}{4}\delta_2$ is remarkable, for I expected a probable accidental error in each value of $\pm 0''{\cdot}25.(\pm 1''{\cdot}7 \div \sqrt{176 \div 6})$. This is perhaps evidence of the superior character of the night observations.

From the mean we have

$$\delta_1 - \tfrac{5}{4}\delta_2 = -0''{\cdot}26$$

and to obtain δ_1 we must adopt a value for δ_2 from other sources—with this inconvenience, that any error in δ_2 introduces a larger error into δ_1.

The truth probably lies between the following two extremes:—

(1) The variation should have its theoretical value or $\delta_2 = +0''{\cdot}39$ leading to $\delta_1 = +0''{\cdot}23$.

(2) The variation should have its value as determined from all the observations, or $\delta_2 = -0''{\cdot}03$ and $\delta_1 = -0''{\cdot}30$, or, in other words, I assume that the variation apparent from the

observations between 8^h and 16^h will be intermediate between the true variation and the variation apparent from all the observations.

Consequently

$$\delta_1 = -0''\cdot03 \pm 0''\cdot27$$

the $\pm 0''\cdot27$ indicating the probable *extreme* limits to the unknown systematic error.

The solar parallax corresponding to $\delta_1 = -0''\cdot03$ is $8''\cdot79$, as found last month.

I take this opportunity of stating that my principal object is to analyse the Moon's errors with a view to obtaining empirical coefficients to be compared with Professor Brown's theoretical ones. From time to time there will be digressions into the values of astronomical constants, accompanied, I hope, by a discussion of the accidental error and a notification of undetermined systematic errors.

Reduction of 295 *Photographs of Eros made at Nine Observatories during the period* 1900 *November* 7–15, *with a determination of the Solar Parallax.* By Arthur R. Hinks, M.A.

§ 1. *Introduction.*—In two previous papers (*Monthly Notices*, 1901 November and 1902 June, vol. lxii. pp. 22 and 551) there is an account of the experimental reduction of certain photographs of *Eros* made at Cambridge, Lick, and Minneapolis. Its object was to test a method of reduction in rectangular coordinates; and the conclusion was that the method was simple, convenient, and worthy of a more extended trial. I therefore ventured to propose that we should undertake at Cambridge the reduction of so much of the photographic material obtained during the period 1900 November 7–15 as might be placed at our disposal by the kindness of the directors of the different observatories taking part in the cooperation to observe the planet *Eros* at that opposition. At the time this proposal was made we had very little information as to the real accuracy of the photographic method when pushed to the limit, and especially little knowledge of what systematic discordances might be found in the work of different observatories. It was hoped that the discussion of the material of these nine days, considerable in itself, but only a small part of the whole, might lead to a preliminary value for the solar parallax of weight equal to that of the best existing values, and at the same time be some guide in the operations which must eventually be undertaken to combine the whole of the observations in one definitive solution.

§ 2. The first step was to choose the stars which were to form the standard of comparison stars.